EXHIBIT F
REDACTED

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CONTAINS CONFIDENTIAL ATTORNEY EYES ONLY INFORMATION SUBJECT TO PROTECTIVE ORDER

UNITED STATES DISTRICT COURT
WESTERN DISTRICT OF PENNSYLVANIA

LAMBETH MAGNETIC STRUCTURES, LLC, Plaintiff, v. SEAGATE TECHNOLOGY (US) HOLDINGS, INC. and SEAGATE TECHNOLOGY LLC, Defendants.	Civil Action No. 2:16-cv-00538-CB Judge Cathy Bissoon **CONTAINS CONFIDENTIAL ATTORNEY EYES ONLY INFORMATION SUBJECT TO PROTECTIVE ORDER**

<u>REPLY EXPERT REPORT OF DR. KEVIN COFFEY TO THE EXPERT REPORT OF DR. ERIC E. FULLERTON</u>

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I. INTRODUCTION

1. I, Kevin R. Coffey, Ph.D., resident at 2364 Foliage Oak Terrace, Oviedo, Florida, 32766, have been retained by counsel for Plaintiff Lambeth Magnetic Structures, LLC ("LMS") to provide my opinion concerning U.S. Patent No. 7,128,988 ("'988 Patent"), including whether certain products manufactured, used, sold, offered for sale, and imported to the United States by Defendants Seagate Technology (US including whether certain products manufactured, used, sold, offered for sale, and imported to the United States by Defendants Seagate Technology (US) Holdings, Inc. and Seagate Technology LLC (together, "Seagate") infringe claims of the '988 Patent, and whether certain products manufactured, used, sold, offered for sale, or imported to the United States by Seagate practice claims of the '988 Patent.

2. I previously provided an expert report in this matter pertaining to Seagate's infringement on May 2, 2018 ("Coffey Initial Expert Report"). On July 16, 2018, Dr. Fullerton Eric E. Fullerton provided an expert report at Seagate's request pertaining to whether certain Seagate products infringe the '988 Patent. My conclusions and opinions contained herein are in response to Dr. Fullerton's July 16, 2018 report. Unless indicated otherwise, the conclusions and opinions in my Coffey Initial Expert Report remain unchanged.

3. The materials I considered in reaching the conclusions and opinions in my Coffey Initial Expert Report were identified in Appendix A to that report. Additional documents and materials I have considered since then are identified in Appendix J, attached to this report.

4. My qualifications, compensation and expert testimony in the last four years were set out in my Coffey Initial Expert Report.

5. The legal standards I used in my infringement analysis are set forth in my Coffey Initial Expert Report and I have applied them herein as well.

6. I am prepared to testify to the matters set out in this report at trial, as well as those set forth in my Coffey Initial Expert Report, as explained therein.

II. DR. FULLERTON'S TECHNOLOGY BACKGROUND AND '988 PATENT DESCRIPTION IMPROPERLY INJECT ADDITIONAL REQUIREMENTS INTO THE CLAIMS

7. In Section V.B. of Dr. Fullerton's Report, he claims to be describing Epitaxial Growth and Variants. To the extent Dr. Fullerton's descriptions conflict with the Court's Claim Construction or otherwise attempts to inject additional requirements into the claims other than those explicitly set forth by the claim language itself, I disagree with Dr. Fullerton's descriptions and opinions. For example, to the extent Dr. Fullerton claims that a layer must be of a certain thickness to qualify as an atomic template (*see*, *e.g.*, Fullerton ¶¶ 64-65), I disagree. Further, to the extent that Dr. Fullerton is suggesting variants must be defined with respect to its orientational relationship with respect the underlying hexagonal atomic template, the Court's Claim Construction of variant(s) contains no such requirement. The Court construed a variant to mean "one of a set of possible crystal orientations." *See* Claim Construction Order, at 8.

8. I disagree with Dr. Fullerton's characterization of "the central feature of the '988 Patent" (*see* Fullerton ¶ 70) to the extent it purports to inject additional requirements into the claims other than those explicitly set forth by the claim language or the Court's Claim Construction.

9. I disagree with Dr. Fullerton's statement at paragraph 65 that "layers that are only a few atomic layers thick often exhibit no long-range crystal structure…" This is not generally the case, and this is very dependent upon the substrate material, the deposited material, and the processing used to generate the deposit.

10. I disagree with several of Dr. Fullerton's assertions in his Technology Background section regarding Measuring the Crystallographic Properties of Materials (Section V.C.). His claims of four-digit accuracy for the ratio of interatomic distances at paragraph 84 and his suggestion of a 3% accuracy at paragraph 85 may be achievable in large grained or single crystal samples but seem unreasonable for thin films wherein strains (bending of the crystal) and

defects routinely distort crystal interatomic spacings. I disagree with Dr. Fullerton's comments in paragraphs 95 through 97 to the extent they suggest an improper construction of any claim term, including "bcc-d layer" or "broken symmetry" and to the extent they suggest that "texture" requires 100% of the same grain orientations.

11. Dr. Fullerton's statement that "FFT images can sometimes be used to infer information about crystal structure and crystal orientation – but only for the specific crystal or crystals being imaged" (Fullerton ¶ 105) is too restrictive, as samples of portions of thin films are often used to infer information about the crystal structure and/or orientation of the entire layer or film because to test and measure every single crystal within a layer would be unnecessary. Sampling is a commonly accepted scientific method of determining characteristics of the entirety of a thin film or layer.

12. To the extent Dr. Fullerton is suggesting that the only way to determine whether a material is uniaxial is by taking magnetic measurements of the material (*see* Fullerton ¶ 85), I disagree. As explained in my Coffey Initial Expert Report at, for example, paragraphs 192-198, I utilized the Chikazumi equation to calculate the anisotropy energy density function, which is well known in the literature and utilized in the '988 patent.

13. To the extent Dr. Fullerton's description of the '988 Patent suggests that there is an "exchange coupling" requirement in the Asserted Claims (*see* Fullerton ¶ 168-169), I disagree. Neither the claim language nor the Court's Claim Constructions provide such a requirement.

14. Further, to the extent Dr. Fullerton suggests that the angles of the variants must be measured with respect to a hexagonal template in order to meet the limitations of the Asserted Claims (*see*, *e.g.*, Fullerton ¶¶ 170-171), I disagree. As explained further below, the variants of the '988 Patent can be seen and measured without reference to a specific hexagonal template.

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III. INFRINGEMENT REPLY

A. The Accused [redacted] Products Infringe

15. I have reviewed and considered Dr. Fullerton and Dr. Stach's July 16, 2018 Reports, and it remains my opinion that all of Seagate's Accused Products Infringe the Asserted Claims of the '988 Patent.

16. Dr. Fullerton spends much of his rebuttal report attempting to rebut the work of Dr. Clark, on which I relied in my Coffey Initial Expert Report. I reviewed the analysis of Dr. Clark prior to the submission of my Coffey Initial Expert Report, in which I relied on his analysis. It is my opinion that Dr. Clark's experimental work as reported in his Clark Initial Expert Report is scientifically and methodologically sound, reliable, and accurate. I have more than 32 years of experience in this field, including approximately 11 total years working at Seagate and IBM (now Western Digital). Where appropriate below, I explain my disagreement with Dr. Fullerton's criticisms of Dr. Clark's work. I also understand that Dr. Clark intends to rebut arguments made by Dr. Fullerton in his own response report. For that reason, I may not rebut all of the arguments made solely against Dr. Clark's work in this report.

1. The Accused [redacted] Products have "at least one layer providing a (111) textured hexagonal atomic template disposed between said substrate and said bcc-d layer"

17. With respect to Dr. Fullerton's criticisms of Dr. Clark's analysis, I disagree with Dr. Fullerton's assertion that Dr. Clark's analysis is flawed and unreasonable for the reasons explained below.

18. I disagree with Dr. Fullerton's assertion that Dr. Clark's analysis is flawed and unreasonable (*see*, *e.g.*, Fullerton ¶ 267) and that he did "not describe the methodology he used to compare the NiFe FFT to the $\{110\}_{FCC}$ diffraction pattern" (*see*, *e.g.*, Fullerton ¶ 271). Dr.

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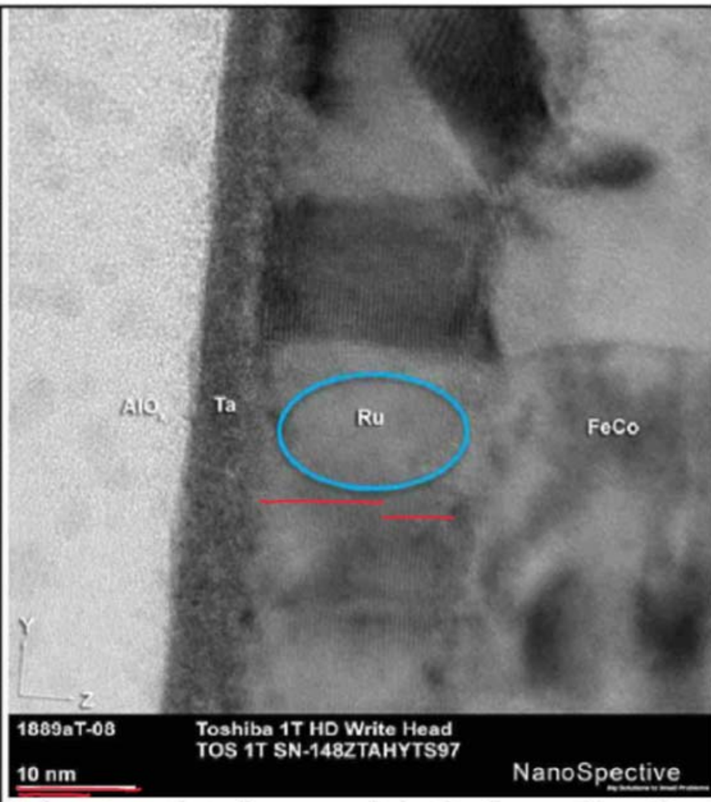
Clark's report provides the FFT diffraction patterns, as well as the standard diffraction pattern rotated to the FFT's orientation from which he determined a match. *See* Clark Initial Expert Report ¶ 89. Dr. Clark also says that "[a]nalysis of the FFTs was performed by measuring the diffraction spot spacings and the angle between rows of diffraction spots and comparing that information with the standard diffraction patterns." Clark Initial Expert Report ¶ 88. This provides an explanation of the methodology Dr. Clark used.

19. I further specifically disagree with Dr. Fullerton's assertion that Dr. Clark's analysis is flawed and unreasonable (*see*, *e.g.*, Fullerton ¶ 311[1]) due to the number of FFT measurements on which Dr. Clark relied to determine that the NiFe layer has a predominate (111) crystal orientation. As explained above, sampling is a commonly accepted scientific method of determining characteristics of the entirety of a thin film or layer. I understand that Dr. Clark examined several spots of each sample and that the micrographs shown are consistent with the layers' characteristics.

20. I disagree with Dr. Fullerton's opinion that Dr. Clark's microbeam diffraction data contradicts his opinion that the NiFe layers are (111) textured. *See*, *e.g.*, Fullerton ¶ 363-370. Dr. Fullerton seems to contend that because Dr. Clark did not identify a hexagonal pattern in the microbeam diffraction measurements, it must not exist. But this is not the case. The (111) textured hexagonal template is still present, even if the hexagonal template cannot be seen in the diffraction images due to the thinness of the NiFe layer. In the "example" which Dr. Fullerton cites, the underlying atomic template layer in Toshiba's accused products was a Ruthenium layer measuring at least approximately 15 nm thick, which are both factors that allow the hexagonal pattern to be easily observed using microbeam diffraction.

[1] Throughout his report, Dr. Fullerton criticisms for each sample analyzed are repeated and my responses herein apply to each similar criticism in Dr. Fullerton's report.



It is a template because it is the layer directly underlying the IronCobalt, which was epitaxially grown thereon.

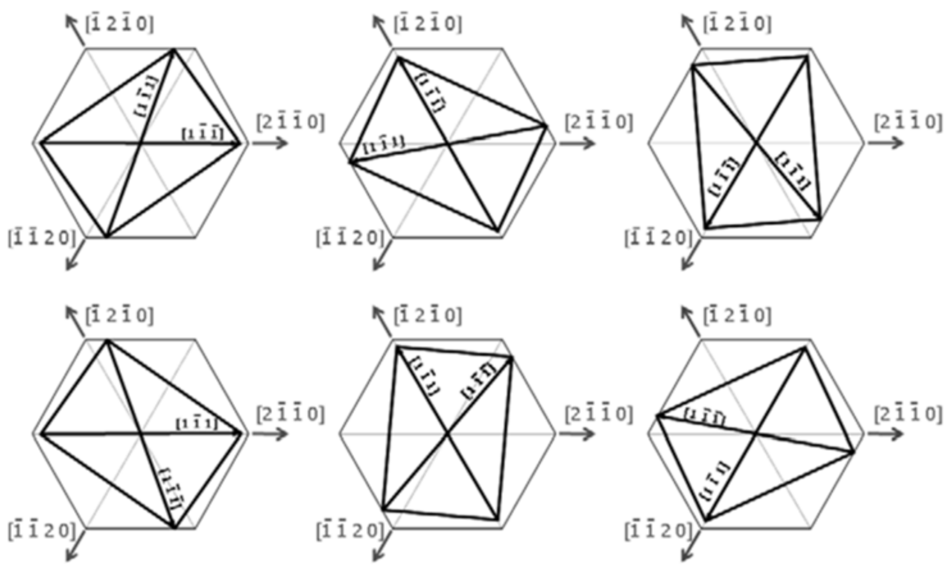
The fact that the Ruthenium layer is a (111) textured hexagonal structure is confirmed by electron beam diffraction analysis as demonstrated by the hexagon in the diffraction image below:

(LMS's Infringement Contentions (served on Toshiba on Oct. 9, 2015), at 9.) In contrast, the [redacted]. *See* Seagate's First Supplemental Response to LMS's Interrogatory No. 10, served May 16, 2017 at 13-14.

21. Further, the existence of the KS variants in the FeCo layer disposed on top of the NiFe layer demonstrates that the NiFe layer necessarily has an FCC (111) texture, because the KS variants would not form and orient at those particular in-plane angles to one another in the

absence of a (111) hexagonal template. I discuss the existence of the KS variants in my Coffey Initial Expert Report at, for example, Section VI.1(d), as well as further in response to Dr. Fullerton's arguments below.

22. Dr. Clark's Initial Expert Report includes a figure demonstrating the six variants of the KS variant system and their orientations relative to a hexagonal template.



Clark Initial Expert Report ¶ 37, Fig. 9. That means that there are specific angular relationships between the six variants as well, as also shown in Figure 9. That is, certain angles exist between the six variants themselves, in addition to the angles relative to a hexagonal template. These angles between the six variants exist only in the KS variant system – not others. The angles between the variants are a "fingerprint" of the six-variant system.

23. The special angles all derive from the angle the variants form with one of the diagonal axes of the variant and the underlying template. That angle is ideally 5.264˚. *See* '988 patent at 17:38. Two variants can lie on the template with double that angle between them, or 10.53˚. This can be seen by comparing the first and sixth variant orientation shown above. Possible acute angles between the [100] directions of any pair of variants include 10.53˚, 49.47˚,

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60˚, and 70.53˚. *See, e.g.*, ’988 patent at 17:48-53. For example, there is a 70.53˚ angle between the first and fourth variants shown above.

24. On the other hand, in a three-variant system, the variants *only* lie at 60˚ intervals, *see* ’988 patent at 16:25-27, and would not lie at the special angles of 10.53˚ or 70.53˚.

25. In his opening report, Dr. Clark shows microbeam diffraction patterns taken from samples having more than one variant. As can be seen from these diffraction patterns, the angle between the variants is not solely 60˚, thus eliminating the three-variant system from consideration. *See, e.g.*, Clark Initial Expert Report. In fact, the angles are not random and the most common angles between variants is around 70˚ or 10˚, which are the angles expected of a six-variant system and a random distribution of angles is not present. Thus, the angular “fingerprint” matches the six-variant system, not the three-variant system or a random, non-epitaxial relationship.

26. Moreover, where one observes a layer with multiple variants of the KS system, its presence provides evidence of the presence of the (111) hexagonal template on which it was grown beneath it because this six-variant system only occurs over a (111) hexagonal template.

27. Further, when measured, NiFe is usually fcc and (111) textured. *See Investigation of structural and magnetic properties of Ni, NiFe and NiFe2O4 thin films*, Jitendra Singh, Sanjeev K. Gupta, Arvind K. Singh, Prateek Kothari, R. K. Kotnala, J. Akhtar, Journal of Magnetism and Magnetic Materials 324 (2012) 999–1005. Although the films in this paper were 100 – 120 nm films, when combined with the other evidence with respect to the NiFe layers in Seagate’s Accused Products, it further supports my conclusion and opinion that the NiFe layers in Seagate’s Accused Products are (111) textured hexagonal atomic templates.

28. Dr. Fullerton presents reports of some examples where a layer is described as being too thin to have a preferred crystallographic orientation (*see*, *e.g.*, Fullerton ¶ 371) but presents no data that this is the case for the NiFe template layer of the Seagate Accused Products

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nor does he provide any arguments as to why this should apply to them. Dr. Fullerton further relies on pseudomorphic growth of the NiFe layer on an amorphous structure to argue that it is not fcc: "One common feature in deposition of ultrathin films is that in many cases pseudomorphic growth occurs over several monolayers before the film converts into its usual crystal structure" and " Dill never described the crystalline structure of the interface layer . . . but the thickness of that interface layer (i.e., 0.4-2 nm) indicates that it was pseudomorphic fcc and could not have been in equilibrium crystalline phase." These are valid observations for pseudomorphism, and it is well known for normally bcc Fe and FeCo alloys to initially grow as fcc on a fcc underlayer. It should be noted the bcc Fe and FeCo alloys have a strong tendency to form in fcc crystal structures, even in equilibrium conditions. For example, pure Fe forms in the fcc phase at elevated temperatures and FeCo alloys (at lower Fe concentrations) are known to form as fcc alloys at room temperature. *See*, *e.g.*, *Fe (Iron) Binary Alloy Phase Diagrams*, ASM HANDBOOK, ASM International, Alloy Phase Diagrams, Vol. 3, 340-61 (2016). Accordingly, pseudomorphic growth of fcc Fe and FeCo alloys is expected. This is not the case for the pseudomorphic growth of amorphous Ni on an amorphous template, as Dr. Fullerton suggests. Pure Ni and Ni alloyed with Fe and/or Co is not expected to form an amorphous phase at room temperature on any template layer and, in contrast to fcc Fe on a fcc template, I am aware of no examples of pseudomorphic growth of amorphous Ni at room temperature or above and Dr. Fullerton has not provided any such examples.

29. With respect to Dr. Fullerton's opinion that Dr. Clark's work shows that FeCo crystals grow irrespective of the NiFe layer (*see*, *e.g.*, Fullerton ¶ 376), I disagree. In fact, Dr. Clark's work demonstrates just the opposite of what Dr. Fullerton claims. The growth of the upper NiFe layer is upon the lower FeCo layer, which directs its growth to a fcc (111) orientation, i.e., it becomes a suitable template layer, directing the growth of the FeCo layer above it. Note that this template layer is necessarily at the same orientation in the plane as the

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template below the lower FeCo layer. I think it is likely that the same variant orientation will form in the upper layer as was present in the lower FeCo films (as observed by Dr. Clark) due to the presence of the same magnetic field breaking the symmetry of both layers. Dr. Fullerton's suggestion that the two FeCo layers would share a crystallographic orientation with a vacuum or air space between them (in the absence of the NiFe layer) is wrong.

30. I reiterate my opinion that strong evidence of an underlying (111) hexagonal template layer comes from the identification of the special angles between Kurdjumov-Sachs orientation. My opinion has been further confirmed by the additional high-resolution images included in Dr. Clark's Reply Report.

2. The Accused [redacted] Products have "at least one bcc-d layer which is magnetic, forming a uniaxial symmetry broken structure"

31. First, I note that Dr. Fullerton admits that the FeCo layers are magnetic bcc-d layers. *See*, *e.g.*, Fullerton ¶ 381.

32. I disagree with Dr. Fullerton's interpretation of the Court's Claim Construction Order as creating additional limitations (*see*, *e.g.*, Fullerton ¶ 382), and contrary to Dr. Fullerton's assertions, I applied the Court's construction of claim terms to my analysis of each limitation.

33. With respect to the Court's construction of "symmetry broken structure," Dr. Fullerton injects additional requirements to the already-construed term that the Court did not require. For example, Dr. Fullerton injects additional limitations for the term "bcc-d variant." The Court construed "variants" to meant "two or more of a set of possible crystal orientations." While Dr. Fullerton acknowledges that construction, he goes on to inject multiple additional requirements to the term. For example, Dr. Fullerton's analysis improperly requires at least three additional "limitations" in order to demonstrate the existence of a "bcc-d variant" – "(1) a bcc-

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d(110) crystal; (2) the underlying (111) hexagonal template crystal on which that bcc-d(110) crystal was grown; and (3) the orientation of the bcc-d(110) crystal relative to the template crystal." *See*, *e.g.*, Fullerton ¶ 376. But the Court's construction of "variants" does not contain these requirements.

34. As I explained above, however, the presence of the bcc-d variants can be detected and measured without a measured angular reference to an underlying template. This is because there are specific angular relationships ***between the six variants themselves***, regardless of whether such angles are ***measured*** relative to the template crystal. These specific angles exists only in the six-variant system; they are a "fingerprint" of the six-variant system.

35. In his Initial Expert Report, Dr. Clark shows microbeam diffraction patterns taken from samples having more than one variant – i.e. two or three variants. As can be seen from these diffraction patterns, the angle ***between the variants*** is not solely 60˚, thus eliminating the three-variant system from consideration. *See, e.g.*, Clark Initial Expert Report at ¶¶110-113. In fact, the angles are not random and the most common angles between variants is around 70˚ or 10˚, which are the angles expected of a six-variant system. The angles between variants, thus, constitute the angular "fingerprint" that matches the six-variant system, not the three-variant system or a random non-epitaxial system, demonstrating the existence of "bcc-d variants of a six variant system."

36. I disagree with Dr. Fullerton's opinion that Dr. Clark's analysis of 100 microbeam diffraction patterns for the FeCo layers "indicates that many (if not most) of the crystals ... are not bcc(110)." *See*, *e.g.*, Fullerton ¶ 455. Rather, it is likely that the images that were not identifiable as bcc(110) are nevertheless bcc(110) crystals. That they were not identifiable as such is due to very slight variations in angles of the crystals in relationship to each other such when they are being imaged. It does not indicate that the crystals are something other than bcc(110), as Dr. Fullerton suggests. Our reports showed that the texture of the bcc FeCo grains

was predominantly (110). But a textured film is not all oriented perfectly in the direction normal to the template. Some grains may be tilted from normal by as much as 10° or more. On the other hand, an electron microscope is quite sensitive to small changes in angle. A crystal whose crystalline axis is off by as much as 1° or 2° may not demonstrate a clear diffraction pattern. It was thus completely expected by Dr. Clark and me that much of the bcc FeCo material would not have a clear microdiffraction pattern because some of the material would not lie in exactly the right orientation to generate such a pattern. That does not mean the material was not (110) textured bcc—just that some of the grains were not observable by that particular technique because of the slight tilt of some of the (110) textured bcc grains. This is demonstrated by the tilting examples included in Dr. Clark's Reply Report. In my opinion, (110) textured bcc crystals are *not* in the minority; in fact, most of the FeCo material is (110) textured bcc, and forms a six variant system.

37. Section IX.A.2(a)(ii) of Dr. Fullerton's Report contains multiple errors in his construction of the claims and injects requirements that do not exist in the claim language or the Court's Claim Construction. First, he alleges that Dr. Clark's microbeam diffraction patterns show more than six "variants" in each FeCo layer and that "[b]ecause a 'symmetry broken structure' requires a 'six variant system,' a layer containing more than six 'variants' cannot form a 'symmetry broken structure.'" *See*, *e.g.*, Fullerton ¶ 467. Again, Dr. Fullerton misapplies the Court's Claim Construction. The Court has held that a "symmetry broken structure" is unequal amounts of bcc-d variants of a "six variant **system**" (emphasis added), and that a variant is "one of a set of possible crystal orientations." The Court's claim construction does not limit the claims to single crystal hexagonal template layers, in which only six possible crystal orientations would exist. *See* Claim Construction Order, at 7-8. As Dr. Fullerton acknowledges, the six variants of the KS variant system are defined by their orientational relationship to a hexagonal template. I note this is however, not the only way to measure or detect the existence of the KS

variants. Because the polycrystalline NiFe material in the Accused Products has multiple hexagonal (111) crystallites in a single layer, each such crystallite has six possible variants that can be epitaxially grown on its surface. Thus, for each crystallite from which microbeam diffraction patterns were taken, there are never more than six variants, and thus they meet the Court's Claim Construction. Dr. Fullerton's error with respect to this claim construction is also evident in his analysis of Dr. Clark's diffraction patterns. Dr. Fullerton's argument at Paragraph 505 that "[t]he continuous rings indicate that bcc-d crystallites exist with many – many more than six different in-plane orientations" again neglects that variants are ***defined*** with respect to a single template crystal and a polycrystalline template layer is present with many in-plane orientations of the template crystals. <u>Second</u>, he suggests that the only way to determine whether a layer has symmetry broken structure is via a pole figure. *See*, *e.g.*, Fullerton ¶ 496. This is not true; it is merely one possible way to do so. Dr. Clark's cross-sectional TEM images establish that the in-plane average grain size of the template layer is similar to the average in-plane grain size of the bcc-d variants upon the template grains, although variations in grain size above and below the average are expected. Accordingly, with this geometry, in many cases a symmetry broken structure will be formed by having a single variant grain on each template grain. In some cases (more likely with the smaller variant grains and larger template grains) there will be two variant grains on a single template grain (as has been observed by Dr. Clark in his microbeam diffraction data). With decreasing likelihood, instances of three, four, five, and all six variants on a single template grain will occur, but these will all be examples of a symmetry broken structure, except for that very rare case in which not only all six variants are present on a single template grain, but they are present in equal amounts, as would be needed to <u>not</u> be symmetry broken. While this, in principle, can occur, Dr. Clark's microbeam diffraction data found relative few instances of two variant grains on a template grain, and rarely instances of three variants, and no examples of four, five, or six variants on a template grain were observed. Dr.

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Clark's dark field imaging techniques are also a way to establish broken symmetry by unequal amounts of variants in different crystallographic directions on a polycrystalline template layer. Dr. Fullerton's suggestion to do x-ray diffraction pole figure studies (as in the '988 patent) is mostly appropriate for a single crystal and, in any case, not appropriate for the Accused Product's write head samples due to the very small area of the write pole bcc-d layer. Instead Dr. Clark used dark field imaging and TEM, both of which are conventional testing techniques.

38. I disagree that assessing whether a magnetic bbc-d layer is uniaxial requires magnetic testing and analysis of magnetic data, as Dr. Fullerton contends. *See*, *e.g.*, Fullerton ¶ 513. For example, NASA claims the mass of the moon is known to four significant figures and they never brought the moon to the earth's surface and put it on a scale. The '988 Patent establishes a single maxima and a single minima, as required by the Court's Claim Construction, by calculation, by adding magnetic energies. The '988 patent teaches how to calculate the energy as a function of angle to determine if a single maxima and a single minima are within 180 degrees. *See*, *e.g.*, '988 patent at 11:30-58. This is the methodology I used in my Coffey Initial Expert Report to establish that the FeCo layers are uniaxial according to the Court's Claim Construction. Particularly due to the requirement that the uniaxiality be as a result of symmetry breaking, proving the uniaxiality of the material through calculations was required in order to avoid other possible contributions.

39. Dr. Fullerton's assertion that "[s]hape anisotropy exerts a significant influence on the magnetic anisotropy of the FeCo layers" (*see*, *e.g.*, Fullerton ¶ 513) is irrelevant to whether the claimed three layer material structure that is present in Seagate's Accused Products is "a structure that is uniaxial as a result the structure being symmetry broken" because shape anisotropy is a property of the device. Independent claim 1 recites "[a] magnetic material structure," and independent claim 27 recites "[a] magnetic device *having incorporated therein* a magnetic material structure." Thus, a "magnetic material structure" is *not* a "magnetic device,"

but is "incorporated" within a magnetic device. Examples of a "magnetic device" are found in the dependent claims, and include "a magnetic data storage system" (claim 30), and "a data storage magnetic recording transducer" (claim 31). The "magnetic material structure" is simply the three-layered structure defined by the claims. That structure has been shown by the inventor (for example in the '988 patent) and by the work performed by Dr. Clark and me (for the accused products) to be uniaxial "as a result of the structure being symmetry broken." Shape and stress anisotropy are irrelevant to the three-layer magnetic material claimed by claims 1 and 27; the magnetic material has been shown to be uniaxial as a result of being symmetry broken.

40. Further, Seagate internal documents confirm the importance of the [redacted]

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[redacted]

41. The importance of [redacted]

42. This can be contrasted with [redacted]

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[redacted]

43. The importance of [redacted]

44. The importance of even [redacted]

[redacted]

45. [redacted]

46. In Section IX.A.2(c) of his report, Dr. Fullerton's criticizes my energy calculations, both with respect to my dark field image data and my values of K_1 and K_2, among others. His criticisms are misplaced.[2] First, Dr. Fullerton alleges that I used data that I "wished Dr. Clark had measured" and that I used "data forced to fit a perfect sinusoidal function." *See*, *e.g.*, Fullerton ¶¶ 552, 560-65. Dr. Fullerton criticizes my fitting the dark field data with a sinusoidal function, rather than using the "empirical data." *See*, *e.g.*, Fullerton ¶ 563. All experimental data has some level of noise and it is standard scientific practice to fit a mathematical model to the data to identify the fundamental pattern of the data as distinct from the noise present in the data.

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[2] I also disagree with Dr. Fullerton's [redacted]

47. Second, Dr. Fullerton criticizes my calculations of energy density because he alleges that my assumed values of K_1 and K_2 are not reasonable. Specifically, Dr. Fullerton believes that assuming K_1 is large and positive and K_2 is equal to zero is unreasonable. Although I disagree with Dr. Fullerton, I have done the calculations using values with K_2 larger in magnitude than K_1, which Dr. Fullerton claims is reasonable, to show that this does not affect my conclusions. I used the values of J.M.C. Coey, MAGNETISM AND MAGNETIC MATERIALS, at 385-86, cited by Dr. Fullerton at paragraph 595 of his report. This text gives anisotropy constants for a bcc alloy of Fe 65% and Co 35%: $K_1 \approx 20$ kJm^{-3} (2×10^5 erg/cm^3) and $K_2 \approx -35$ kJ m^{-3} (-3.5×10^5 erg/cm^3). Note that these values assume a non-zero value for K_2 that is *larger* in magnitude than K_1. Nevertheless, as detailed below, this does not change any of my conclusions regarding the material structure within Seagate's Accused Products having a single maximum and a single minimum in magnetic energy as a function angle over 180 degrees of rotation.

48. At paragraphs 591 to 596 of his report, Dr. Fullerton provides alternative reports of the values of the cubic anisotropy constants, K_1 and K_2, bcc for FeCo alloys. The work of Muhge et. al. ("Structural and magnetic studies of FexCo1-x (OO1) alloy films on MgO (001) substrates," Journal of Applied Physics, Vol. 77, page 1055, 1995) reports only the value for K_1 in Figure 7, reproduced by Dr. Fullerton in his report at paragraph 591. The Muhge work provides a K_1 value of approximately -20 kJ/m3 (-2.0×10^5 erg/cm3) for alloy thin films of compositions near Fe 65% and Co 35%. Dr. Fullerton also refers to the authoritative text, MAGNETISM AND MAGNETIC MATERIALS by J.M.D. Coey, which on pages 385 and 386 gives both anisotropy constants for a bcc alloy of Fe 65% and Co 35%: $K_1 \approx 20$ kJm−3 (2.0×10^5 erg/cm3) and $K_2 \approx -35$ kJ m−3 (-3.5×10^5 erg/cm3).

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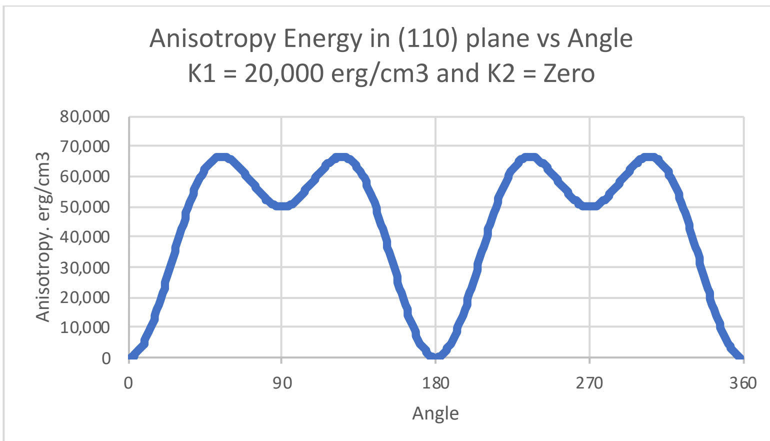
49. The Mughe report is clearly unreliable as a basis for determination of the anisotropy constants appropriate to the bcc-d layers of infringing Seagate products for several reasons:

- The K_1 value of approximately -20 kJ/m3 is very different from the published value of others, including Hall (cited in Coffey Initial Expert Report) and Coey (also cited by Dr. Fullerton).
- The K_1 values were determined from MOKE hysteresis loops, rather than the more reliable torque magnetometry method.
- The samples used were (001) oriented thin film layers, rather than the (110) oriented layer samples of Hall and of the infringing bcc-d magnetic layers.
- The samples used were epitaxial films on a single crystal substrate which can be expected to provide stress anisotropy in the sample plane to confound the desired measurement of the magnetocrystalline anisotropy.
- The scientific quality of the work is suspect in that it claims to include in figure 7 bulk values for K_1, "The solid line reproduces the bulk behavior (Ref. 14)." However, these bulk values are not consistent with other reports and the reference 14 upon which Muhge relied (Ref. 14, T. Nishizawa and K. Ishida, Bull. Alloy Phase Diagrams 5, 250 (1984)) does not provide any magnetic anisotropy data to support figure 7 of Mughe.

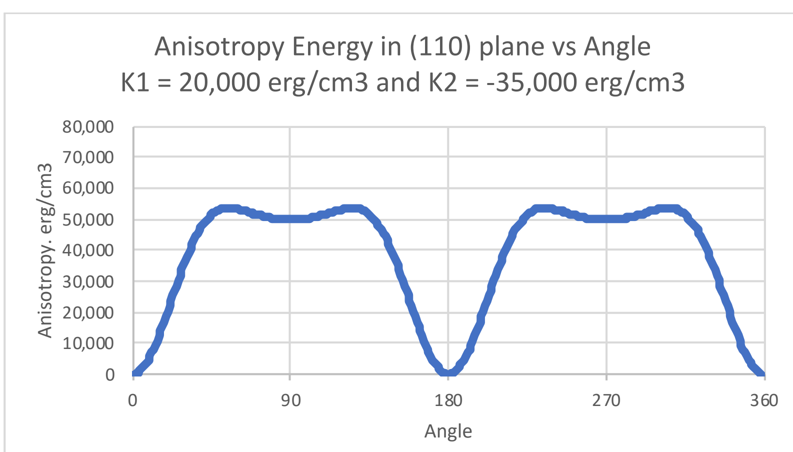
50. The Coey report for both the anisotropy constants for a bcc alloy of Fe 65% and Co 35% can be considered reliable. Although the means by which the values were determined is not given in the text, the K_1 value of $\approx$ 20 kJm−3 (2.0 x 10^5 erg/cm3) is consistent with the work of Hall. There are no other sources than Coey for the K_2 value of ≈ -35 kJ m−3 (-3.5 x 10^5 erg/cm3) for alloys of this or similar compositions that are known to me, so this value can be considered an improvement on the work of Hall and be adopted for further calculations of the

energy density as a function of angle for the Seagate Accused Products. As detailed below, this does not change any of my conclusions regarding the material structure within the infringing products having a single maxima and a single minima in magnetic energy as a function of angle over 180 degrees of rotation.

51. The figure below illustrates the magnetocrystalline energy in the substrate plane as a function of angle in the plane for (110) oriented single variant bcc-d crystal using the K_1 value of 20 kJm−3 (2.0 x 10^5 erg/cm3) and neglecting the K_2 contribution.



52. The figure below illustrates the magnetocrystalline energy in the substrate plane as a function of angle in the plane for (110) oriented single variant bcc-d crystal using the K_1 value of 20 kJm−3 (2.0 x 10^5 erg/cm3) and not neglecting the K_2 contribution and instead including the K_2 term with the value of $K_2 \approx -35$ kJ m−3 (−3.5 x 10^5 erg/cm3) as reported by the Coey text.



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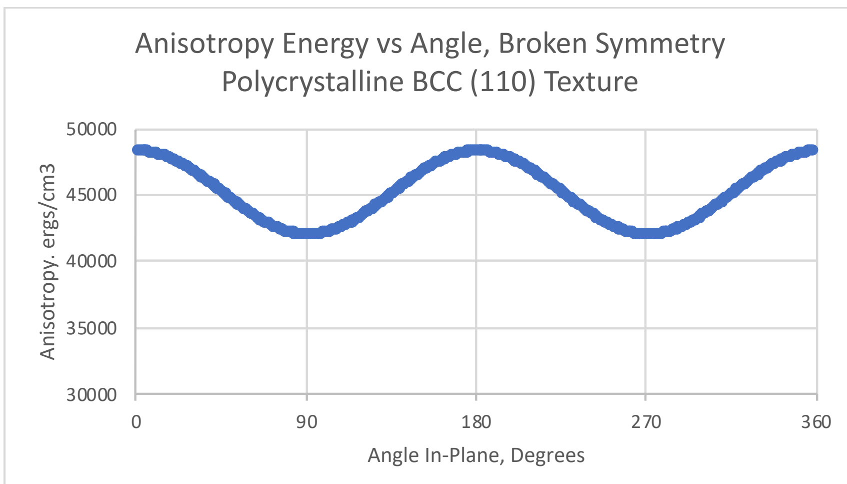
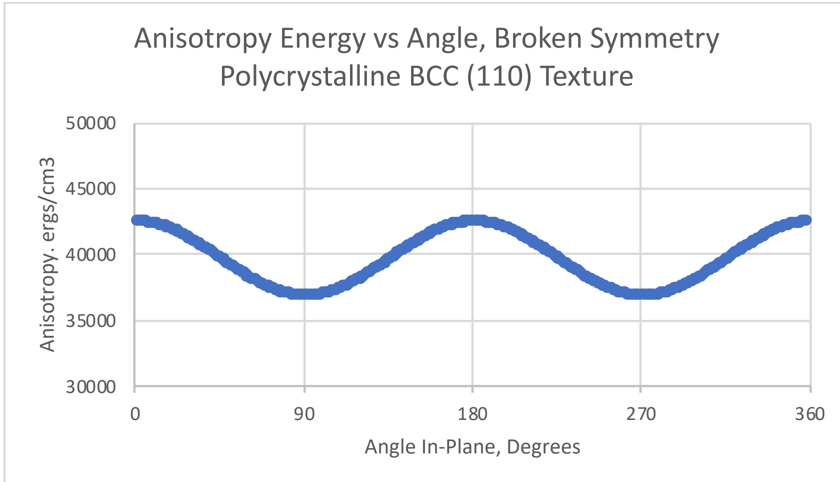
53. An overall reduced anisotropy energy is observed when the K_2 term is included, but the qualitative shape of the energy versus angle function remains similar. In spite of the higher magnitude of the K_2 prefactor (3.5 versus 2.0) the K_2 term can be seen to only result in a small correction of 10% to 15% to the anisotropy energy. This illustrates the decreasing importance of the successively higher order terms, which is the basis for neglecting the K_3, K_4 and even higher order terms.

54. The figure below reproduces the magnetic energy calculated in my prior infringement report for sample SOGPPC, in which the K_2 term was neglected and a K_1 value of 2.06×10^5 erg/cm^3 was used.

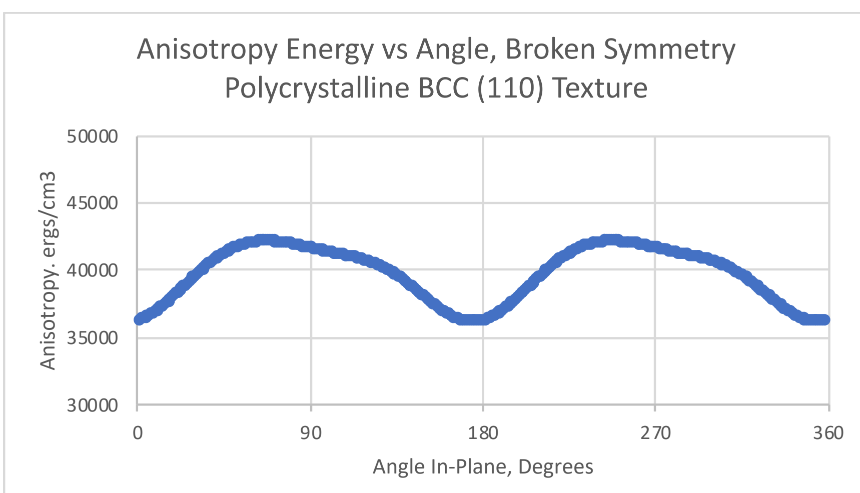


55. This can be compared to the calculation for the same sample using the K_1 value of 2.06×10^5 erg/cm^3 and not neglecting the K_2 contribution and instead including the K_2 term with the value of $K_2 = -3.5 \times 10^5$ erg/cm^3 as reported by the Coey text, which is shown below.



56. A comparison of the two figures, with and without the K_2 term, clearly shows that the fundamental conclusion is unchanged, that the sample exhibits uniaxial anisotropy due to symmetry breaking.

57. In response to criticism of my fitting function (*see*, *e.g.*, Fullerton ¶¶ 552, 560-65), I also recalculated the energy density functions by dispensing entirely with the fitting function and using the raw data points alone. This includes all of the “peaks” and “valleys” of the raw crystallographic data. For these calculations, I used K_1 (2.06 x 10^5 erg/cm^3) and K_2 values (−3.5 x 10^5 erg/cm^3) as above. This is shown in the figure below for the same samples as in the paragraph above.



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58. As is apparent, without the fitting function, based on the crystallographic orientation data directly, the sample has a single maxima and a single minima of magnetic energy in 180 degrees, i.e., is uniaxial. I also point out that this energy density curve has an unphysical shape, due to the noise in the data, in that it lacks symmetry about the easy and hard axis directions. The use of a magnetic field as a symmetry breaking mechanism would not be expected to cause a different shape of the energy curve to the left or right on the energy minima. Hence the use of the fitting function provides a more correct variation of magnetic energy with angle. I also did this calculation for the other samples of my infringement report. For every sample, using this analysis, that includes the noise contribution, I found uniaxial behavior without using a fitting function.

59. Dr. Fullerton's suggestion that I was required to demonstrate exchange coupling is in error. *See*, *e.g.*, Fullerton ¶ 597. There is not an "exchange coupling" requirement in the language of the Asserted Claims or in the Court's Claim Constructions.

B. The Accused [redacted] Products Infringe Claims 1 and 27

60. As explained in my Coffey Initial Expert Report, I was unable to obtain a sample of an [redacted] Product to reverse engineer. However, because the [redacted] Products are substantially similar to the [redacted] Products which I did reverse engineer, I am able to rely on that reverse engineering in support of my infringement analysis for the [redacted] Products. *See* Coffey Initial Expert Report, ¶ 460. Dr. Fullerton alleges that the entire layer stacks are not "substantially similar," ignoring that my analysis focuses on the lower NiFe and FeCo layers and the similarities of the [redacted] Products with respect to those layers. Thus, Dr. Fullerton's criticisms do not change my analysis.

IV. ANALYSIS REGARDING DAMAGES ISSUES

61. In paragraphs 858-868, Dr. Fullerton describes the various components of a PMR HDD and the care that must go into designing, manufacturing and/or optimizing each of these

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components. Even if everything Dr. Fullerton says is correct, that is irrelevant to the opinion expressed in my opening report that the ’988 Patent was critical to mitigating erase after write and hence Seagate’s commercialization of PMR HDDs.

62. In paragraph 869, Dr. Fullerton comments that “there are many other ways to achieve higher density without using higher coercivity media.” Regardless of whether this is true, it has little or no relevance to the products at issue in this case, which do use higher coercivity materials. It should also be noted that the “many other ways” would also benefit from higher coercivity media to provide even higher density.

63. In paragraphs 870-877, Dr. Fullerton summarizes, criticizes and disagrees with my opinion about the significance of the ’988 Patent to PMR HDDs and the EAW problem. I stand by my original opinion; the ’988 Patent was critical to Seagate’s successful commercialization of PMR HDDs as described in detail in my opening report.

64. In paragraphs 879-887, Dr. Fullerton opines that he believes that U.S. Patent No. 4,949,039 relates to technology that is comparable to the ’988 patent and was a “vastly greater improvement than the invention claimed in the ’988 patent.” I have reviewed his opinions, and maintain my opinion that the ’039 Patent is not technologically relevant because it would have required substantial additional development time and effort before it could be implemented into commercial hard disk drives, and it does not disclose the need for, or methodology for, read heads to be linear through hysteresis reduction and elimination of domain wall movement, which was a necessary technological development during the transition from AMR to GMR. Moreover, Dr. Fullerton’s opinions about the acclaim that the ’039 patent received do not alter my opinion that the ’988 was critical to the commercialization of PMR HDDs.

65. In paragraphs 888-896, Dr. Fullerton addresses the Censtor patents. As I indicated in my opening report, I do believe that these patents are marginally technologically relevant. I do not agree, however, that these patents offer a significantly greater improvement

than the ’988 patent as to PMR technology given the critical nature of the ’988 Patent to Seagate’s successful commercialization of the PMR HDDs. I note that Dr. Fullerton does not really compare the advances of the Censtor patents to the advances of the ’988 Patent, but instead comments on what they relate to, for example, with regard to the ’450 patent, Dr. Fullerton says that “the ’450 Patent describes the geometry of the entire read/write head, including many magnetic thin film layers, and also describes the process for manufacturing these layers. But simply because there are many things described in the patent does not make it greater advance in the technology, and Dr. Fullerton performs no analysis of the relative contributions of the ’450 Patent as compared to the ’988 Patent.

66. In paragraphs 897-904, Dr. Fullerton opines about licenses that Seagate allegedly received rights to use certain patents, including other Lambeth patents, via Seagate’s sponsorship of the DSSC at Carnegie Mellon University. I note that I agree that Dr. Lambeth made many other significant contributions to the current state of the art but these other contributions only serve to bolster my opinion about the importance of the contribution of the ’988 Patent and Dr. Lambeth’s contributions.

67. In paragraphs 905-908, Dr. Fullerton generally discussed patents that were licensed to Seagate by Syndia. I note that Dr. Fullerton indicates that there were 33 patents covered by this license, but only calls out one in his report, U.S. Patent No. 4,702,808. This patent does not mention any application of its methods in HDDs, nor does it reference enhancing areal density of HDDs. Dr. Fullerton also does not explain how the methods of the ’808 patent allegedly contribute to increased areal density of HDDs. It is my opinion that this patent is not relevant to magnetic recording as it describes an apparatus and process for inducing chemical reactions by high power lasers. I know of no examples using such an apparatus and process in the field of digital magnetic recording.

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68. In paragraphs 909-910, Dr. Fullerton discusses patents licensed to Seagate in the White license, but again only mentions one patent, U.S. Patent No. 4,673,996, which he opines in technically comparable to the ’988 Patent. I understand that the damages experts agree that this license is not relevant to the damages analysis, and hence offer no further opinion here but reserve the right to opine further regarding these patents in the event that this license becomes relevant.

69. In paragraphs 911-912, Dr. Fullerton comments on my opinion regarding the Dual Stripe MR Technology. Dr. Fullerton claims that I do not in any way identify the Dual Stipe MR Technology, but this is wrong. My opening report cites many exemplary documents that describe the technology.

70. I understand that Seagate’s damages expert indicates that he had a conversation or conversations with Dr. Fullerton in which he reports that Dr. Fullerton said: (1) to the extent that the ’988 Patent was a “bottleneck patent,” there were thousands of other “bottleneck assets” as of 2006 (July 16, 2018 Rebuttal Expert Report of John C. Jarosz (“Jarosz Report”), ¶ 16); (2) The two most important features that needed to be addressed to enable PMR were the design of media components and the geometry of the write head, neither of which is addressed by the ’988 Patent (Jarosz Report, ¶ 170); and (3) The ’988 Patent does not solve issues associated with “erase after write” (or “EAW”), whereby data are unintentionally erased after being recorded on a disk. In fact, many other technologies can be and have been used to address EAW (Jarosz Report, ¶¶ 153, 321). As to (1), I note that there is no indication of what the alleged thousands of other bottleneck asserts were in 2006, so I cannot respond to this statement in a quantitative manner, but I can reiterate my opinion that the ’988 Patent was a critical development. As to (2) and (3), for the reasons set forth in my opening report and in this rebuttal, I disagree with these opinions. I do not dispute that the design of the media and the geometry of the write head were important, but this does not change my opinion about the significance of the innovations in the ’988 Patent to Seagate’s successful

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Dated: August 3, 2018

Respectfully Submitted,

Dr. Kevin R. Coffey